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THE LINKAGE BETWEEN TECHNOLOGY, DOCTRINE, AND WEAPONS INNOVATION:
EXPERIMENTATION FOR USE

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PREFACE

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I. INTRODUCTION

The theme of this paper is that the linkages between technical innovations and their incorporation in weapons and doctrine are strongly dependent on experiments that generate information about possibilities. My main argument is that a crucial step is required between the output of organized R&D and the formation of doctrine and weapons procurement decisions. This step involves *experimentation in the use of the product.* Today, when systematic, organized R&D can be so successful in producing a dazzling variety of potential technologies, subsystems, systems, and assorted improvements to existing weapons, the choice among these is the critical problem. Questions surrounding the *use* of these new things are loaded with the very strong uncertainties that we have come to accept in the technology development process itself. An experimental approach to use is a necessary concomitant to the successful incorporation of technology and to the derivation of doctrine that will govern its use. Indeed, the notion of innovation is just as aptly applied to *use* as it is to *technology*. The linkages between technology and use, therefore, require careful and explicit attention.

This line of reasoning extends the views introduced by Elting Morison in his historical treatment of civil and military technology in the United States.[1] Morison describes several stages in the development of technology. The first stage, from the late 18th to the late 19th century, centered directly on making things. In the absence of established engineering knowledge and practice, great uncertainties attended the construction of useful products: canals, bridges, railways, turnpikes, steel-mills, etc. The project itself was the experiment, and the great historical projects were the classrooms of a budding generation of self-taught engineers as well as the source of abstracted engineering knowledge. The second stage began with the recognition that research could be applied to the making of things

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[1]Elting E. Morison, *From Know-How to Nowhere, The Development of American Technology*, Basic Books, New York, 1974.

before the product itself was engineered in its final form. The creation of the prototype industrial research centers, first in the chemical firms of Germany, and later at General Electric, Westinghouse, and other companies in the United States signalled the attainment of the new stage. Experiments in science, technology, and prototype products preceded product development.

Morison concluded, however, that the richness of the technological opportunities, created by the very success of the process, carried with it the seeds of confusion. He described, for example, how the United States Navy reacted to the flow of new things by building a wide variety of ships without any unifying idea, and by convening over one thousand technical boards between 1865 and 1890 to consider the vast array of possibilities.[1] Morison then notes that Mahan's studies of naval history and the innovative doctrine derived from those studies provided the theory and rationale of *how* Navies should be *used*. "For the first time in half a century, men had a clear idea of what they were trying to do with their mechanical structures and how they might shape and use them in support of their purpose."[2]

The many different fragmentary notions of how to use the emerging naval technology was evidence of a state of "strong uncertainty." Correct strategies for dealing with strong uncertainties in the doctrine and use of new equipment are necessary to effectively capitalize on new technologies. In this paper, I first explore the concept of "strong uncertainty," and then emphasize the importance of the process of dealing with it. I shall describe examples of successful and less successful attempts to deal with the uncertainties of use; and finally suggest a variety of experimental means for reducing these strong uncertainties, and also consider some policies to implement strategies of experimentation in the use of new systems and technologies.

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[1] Morison, *op. cit.*, Chapter 8; particularly pp. 155-156.

[2] *Ibid.*, p. 159.

II. TYPES OF UNCERTAINTY

Key features of innovative behavior are the amount and kind of uncertainty faced by innovators, and how they deal with it. It is analytically useful here to define several classes of uncertainty, ranging from the certain to the chaotic.[1]

Class I.	Certainty.
Class II.	Probability distributions of known form embedded in known models, covering known possible states.
Class III.	Probability distributions of unknown form embedded in known models.
Class IV.	Uncertain models (strong uncertainties).
Class V.	Chaos.

We can note to begin with that R&D and innovation are concerned with strong uncertainties, but let us consider the other states in sequence of increasing uncertainty. Certainty describes the world of neoclassical economics, classical physics, and much engineering practice. The analytical techniques applied to this stage include the solving of systems of equations (the model), typically through the application of optimization, maximization, or equilibrium conditions. Large bureaucracies--whether in government, business, or the military--perform well here; structures and rules are clear and slow to change. Detailed procedures and regulations can be sufficient to ensure effective behavior. Such a world may be complicated, but it can be dealt with by established techniques.

The addition of probability distributions to the key parameters of models converts them into Class II levels of uncertainty. Statistics and probability techniques are used in the analysis of events, often by transforming the uncertainties into certainty-equivalents. Operations research, systems analysis, and decision-theory were developed to handle this state. Bureaucracies can still be effective, especially when their operations extend over large numbers of events. Insurance is the standard social technique for dealing with the state

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[1]These ideas are stimulated by the work of Burton H. Klein. See, for example, *Dynamic Economics*, Harvard University Press, 1977.

of known probabilities.

The third class of uncertainty exists when the probability distributions are themselves uncertain. Bayesian statistical techniques are used for experimentation and sampling in order to convert this state of the world back into one of better-known probabilities. When experiments are few, the time pattern of events may become relevant, as learning then depends on the outcomes of experiments. Systems analysis and operations research are sometimes useful in showing where more information is needed. Typical military techniques for fleshing out the shapes of probability distributions include operational intelligence and reconnaissance.

However, a different pattern of behavior is called for when key variables can only be guessed at; when only fragments of theories exist; when considerable ambiguity is the norm; when we possess just hints or clues of possible relationships. These are some of the features of the class of strong uncertainties. Here, actors probe intriguing phenomena rather than establish the parameters of distributions. Experiments are typically surprising rather than just information-gathering exercises. Large portions of provisionally held hypotheses may have to be discarded. Broad strategies rather than detailed plans are the appropriate means for dealing with strong uncertainties; examples of broad strategies include weapons development approaches that emphasize parallel projects, sequential decisionmaking, and prototype testing. Effective operation in a state of strong uncertainty requires the generation and use of feedback to modify one's view of the world and how one will act in it, which in turn demands a flexibility of mind and behavior.

Chaos exists when no regularities or consistencies are found between information and theories. Either the world is truly chaotic, or the theories may be wrong. Responses to chaos may be myth and superstition (non-scientific theories), withdrawal to a more regular microcosm (pulling your head under the blanket), living for the moment, madness. If the chaos is really due to bad theories, then diversity in the generation of alternative theories may turn up one that fits (i.e., "explains") the heretofore chaotic events, thus transforming

chaos into a lower order of uncertainty.

In warfare, Generals Patton and Marshall, and B. H. Liddell Hart viewed open, mobile military operations as resembling what has been defined here as the state of strong uncertainty. Marshall, for example, described his dismay upon taking command of the infantry school at Fort Benning in 1927. Amidst "the even tenor of their theoretical ways," classroom battles were organized and predictable.[1] He found officers "had been taught an absurd system, which proved futile the moment a normal situation of warfare of movement arose."[2] Marshall sought to train his officers to "solve problems rather than to memorize rules." "The art of war has no traffic with rules, for the infinitely varied circumstances and conditions of combat never produce exactly the same situation twice."[3]

According to this view, the unit commander is clearly acting under strong uncertainty. The individual soldier, however, in the middle of the shooting, is often in a state of chaos; the grand strategists sitting far from the battle perceive the scene as events in a probability distribution. Military bureaucracies, though, often plan as though the world were certain. A strategy of mass, exemplified by Soviet practice and their concepts of battlefield control, attempts to convert warfare from behavior under strong uncertainty to actions governed by the rules of insurance, or better yet, to a state of certainty. Uncertainties and surprises are met by throwing more forces into the battle, where, it is hoped, the law of averages will prevail. Control is maintained by battlefield algorithms.

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[1] F. C. Pogue, *George C. Marshall: Education of a General,* The Viking Press, New York, 1963, p. 251.

[2] *Ibid.*

[3] R. F. Weigley, *The American Way of War: A History of United States Military Strategy and Policy,* Macmillan Co., New York, 1973, p. 215. It is revealing to note that Marshall established his reputation in World War I in the management of logistics where statistical decisionmaking is appropriate. However, when confronted by the requirements of small-unit operations, he revised his strategy for dealing with a qualitatively different kind of uncertainty.

III. DEALING WITH STRONG UNCERTAINTIES

Successful strategies for dealing with strong uncertainties recognize the risks involved and the overwhelming possibilities of setbacks and mistakes; plans should therefore allow for the likelihood of change as information begins to flow from the innovative endeavors. It is not easy to pick winners in advance, and system planners must maintain a flexibility that the uncertain conditions demand.

The focus of planners and managers under strong uncertainty should be on the *process* of generating and using information, rather than on any specific organizational structure or detailed plan. Often, though, the structure of successful innovators has been copied in vain attempts to duplicate their success, when, in fact, the structure was the effect and not the cause of the desired behavior. An example of this focus on structure rather than process was the Soviet Union's creation of joint research and production enterprises in civilian ministries in an attempt to achieve the R&D outcomes of the military sector. The closeness of R&D to production in military products was only a minor contributor to the success of weapons R&D. Another illustration of a mistaken focus on structure rather than process is provided by the attempts of the French government to promote high technology industries from the mid-1950's to the mid-1970's.[1] The policy, as implemented in the computer and electronics industry, had several dimensions: the fusion of small firms into larger organizations that would dominate their markets; the support of French exports in less competitive markets (Eastern Bloc and Third World); and creation of internally protected markets and outright subsidies.[2] Another strand of this policy was for France to become independent of outside suppliers: in computers, the entire system would be designed and produced in France itself.

All of these moves were counter-productive. The protective

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[1]John Zysman, "Between the Market and the State: Dilemmas of French Policy for the Electronics Industry." *Research Policy* 3 (1975), p. 315.

umbrella reduced the pressures on French firms to match international developments. Rather than compete on a price and quality basis, the policy encouraged firms to follow a political as well as an economic strategy. The products the government desired were not those the firms would have chosen if facing market demands.[1] Furthermore, the advantages of size were not supported by statistical analyses of the relationship between R&D activity and industrial concentration; subsequent research showed that the effect of increased seller concentration on the volume of industrial R&D is likely to be perverse.[2]

The policy for electronics and computers was repeated in atomic energy, high-performance aircraft, space technology, and other areas identified by the government as critical. In very few cases did the policy achieve the desired goals. Paradoxically, the effort "may only have perpetuated a traditional industry looking to the state and the French market rather than toward its competitive situation in a world industry."[3] Additionally, participation across too broad a front forced programs to operate on narrow margins where financial or technological setbacks caused later cancellation or severe cutbacks in government support.

One of the sources of failure of the French policy was the attempt to duplicate the *structure* of American success, rather than the *process*. Large size, integrated system production, and even government support were more the result rather than the cause of American success. What seems to have been overlooked by French policymakers were the motivations of profit-making opportunities and a tremendous amount of uncertainty in the life cycles of firms and the people associated with

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[1] Many of the original small firms had, in fact, competed successfully in world markets by specializing in components rather than in complete systems.

[2] In the French industry "Electrical and electronic measuring and computational equipment," research expenditures as a percentage of sales is 240 percent greater in firms with less than 500 people than in firms with 2000-5000 people. William James Adams, "Firm Size and Research Activity: France and the United States." *Quarterly Journal of Economics*, August 1970, Table IV, p. 397.

[3] Zysman, *op. cit.*, p. 326.

and affected by them.[1] This, of course, is not new. Klein shows similar patterns of highly volatile firm growth and decline for the early years of many new products -- automobiles, transport aircraft and engines, and semiconductors. For example, of the 181 companies that produced automobiles between 1903 and 1926, firm mortality data indicate:

28 percent lasted 3 years or less;
49 percent lasted 6 years or less;
36 percent lasted 10 years or more;
19 percent lasted 16 years or more.[2]

Of the top ten automobile manufacturers in 1903, only two remained in those ranks six years later.

The purpose of this excursion into R&D policy is to demonstrate that the strong uncertainties of innovation must be faced, together with all the potential for unplanned outcomes. Firms in the United States electronics and computer industries in recent times, and in most new industries in the past, first confronted technical uncertainties in bringing their products to market, and then dealt with feedback from the market experiment as users investigated the use of the product. Some firms and many products never made it past the first stage. But, increasingly, as firms have become experienced in the process of R&D, failure is more likely to occur in the marketing phase. Despite the growth of market research, which attempts to simulate and predict market response, product development continues to be a risky business. Mimicry of market structure is not a substitute for this kind of market experiment. Military product development is beset by these problems in extreme form. Clear tests of profitability do not exist. Even when weapons have been used in war, the results may often be ambiguous. The next section considers the case of weapons development.

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[1] Robert G. Gilpin, Jr., "Science, Technology, and French Independence," in T. Dixon Long and Christopher Wright (eds.), *Science Policies of Industrial Nations*, Praeger, New York, 1975, p. 124.

[2] Cited in Klein, *op. cit.*, p. 99.

IV. IMPEDIMENTS TO MILITARY INNOVATION

Decisions to develop a weapon with specific characteristics, to procure it, and to use it in a certain way in combination with other forces, according to a doctrine that governs its deployment are fraught with uncertainties. Moving onto uncertain ground is dangerous and difficult. The dangers arise from the consequences of incorrect decisions. The difficulties arise from informational ambiguities, organizational rigidities, and uncooperative technologies.

The value of innovative weapons can remain ambiguous for many years, despite the evidence of war -- even many wars. Interpretation of the experience of decades of actual combat experience with machine guns, for example, was clouded by conflicting evidence. The *mitrailleuse*, the early multiple-barrel French machine-gun, had been generally acknowledged to be a failure in the Franco-Prussian War of 1870-71, partly because it was used as an artillery piece without the appropriate range; but evaluation of that war had also to acknowledge inept French generalship.[1] Omdurman in 1898 showed the machine-gun deadly against undisciplined tribesmen, but how would it perform against trained European troops?[2] The Boer War of 1899-1901 showed the Maxim machine-gun to be useless when used (sparingly) as artillery against aimed fire from dispersed troops. The contrary experience from the Russo-Japanese War (1904/1905) did little to remove the confusion surrounding the best use of machine-guns -- a confusion that was not relieved until the bloody experience on the Western Front in 1915.

A similar story could be told about American tank design and evaluation in World War II. Faith in the M4 Sherman " was based on a

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[1] Bernard Brodie, "Technological Change, Strategic Doctrine, and Political Outcomes," in Klaus Knorr (ed.), *Historical Dimensions of National Security Problems*, University Press of Kansas, 1976, pp. 286, 291.

[2] Christopher Harvie, *Technological Change and Military Power in Historical Perspective*, Adelphi Paper No. 144, International Institute for Strategic Studies, London, 1978, p. 6.

surprisingly limited amount of experience in tank versus tank combat."[1] In the one major action between German and American tanks in North Africa at the Kasserine Pass, the United States forces suffered a serious defeat, but Army leaders attributed it, quite accurately, to lack of experience. On balance, the Sherman compared well with the early German Mark IV models, but the Tiger was a different matter. However, in its first use, the Tiger was defeated by small antitank guns, owing, however, to poor tactics, terrain, and scarcity rather than to its quality. In Sicily, defeat of German Tiger tanks was aided by naval gunfire and faulty German tactics. "There was no convincing demonstration that American equipment was inferior."[2] Salerno, Italy, and Normandy produced conflicting evidence: poor tank country, quantitative United States superiority, changing tactics and experience, and a mixture of victories and defeats left the Americans convinced that the Sherman was, according to the Chief of Ordinance, "the best tank on the battlefield."[3] Even after engagements with the German Panther, which was almost invulnerable to the Sherman except at close range, American tankers stuck to their belief in the pre-eminence of their weapon. Not until late 1944 did the weight of accumulating operational experience demonstrate the need for a new American design--a need, incidentally, that the Germans had recognized in 1941 when they first met the Russian T-34, to which they responded by design of the Panther. By late 1944, despite the urgent demands of Generals Eisenhower and Marshall, it was too late to develop and deliver a superior American tank to the European theater.[4]

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[1] Charles M. Bailey, *Faint Praise: The Development of American Tanks and Tank Destroyers During World War II*, Ph.D. dissertation (History), Duke University, 1977, p. 85.

[2] *Ibid.*, p. 86.

[3] Quoted by Bailey, *op. cit.*, p. 87.

[4] Arthur J. Alexander, *Armor Development in the Soviet Union and the United States*, R-1860-NA, The Rand Corporation, Santa Monica, California, 1976, pp. 97-99.

Following World War II, debates about the appropriate use of rifles, in American military service for 150 years, were not resolved by recourse to the recent combat experience. Despite the arrival of operations research on the scene and the nearly unanimous analytical support for unaimed firepower, some commanders could point to their own positive experiences with long-distance, aimed fire in the marksman tradition, while other commanders based their arguments and biases--not on the combat usefulness of short-range, and large volume of fire as demonstrated by analysis--but on *their* particular experiences. These debates culminated in the controversy over the selection of the M14 and M16 rifles.[1]

The ambiguities of experience and analysis, however, eventually become resolved by time and the weight of accumulated evidence. Less easy to resolve are the rigidities of organizations. Large, complex organizations such as military services have many understandable, and sometimes even laudable, reasons for resisting innovations. Often enough, though, the reasons are obscure and the results disastrous. Large, complex organizations do not spring up overnight, nor are they designed and built in one piece. They evolve gradually in a very complicated process that owes more to historical accident than to rational design. Large investments in equipment, training, and the mastery of existing doctrine produce inertia to change. Within the organization, information has to be coordinated if it is to be of use. Communication channels must be created. The efficiency of the channels can be increased by the use of codes that compress the volume of information to be transmitted.[2] Understanding and managing internal communications, and learning the codes require considerable investment by individuals. Organizations, once created, have distinct identities because changing the codes or rechanneling the information is costly. The value of firm-related experience to employee productivity is a common observation

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[1] Thomas L. McNaugher, "Marksmanship, McNamara and the M16 Rifle," *Public Policy*, Winter 1980, especially pp. 12-14.

[2] These ideas are taken from Kenneth J. Arrow, *The Limits of Organization*, W. W. Norton, New York, 1974, Chapter 3.

of labor economists. But, more important for our purpose, behavior patterns of the organization itself may persist over long periods because of the investment in the routines and techniques that the organization develops to go about its business. Large oil companies, for example, have persisted with identifiable corporate strategies over 50 years or more. Some are noted for their exploration abilities, others for refining efficiency, or marketing, or finance. These strategies apparently originated in early successes of the firms (often the accomplishments of single individuals) that continue to color and shape behavior decades later.[1]

Persistent behavioral patterns also reflect the primary functions of the organization. Behavior appropriate to the primary functions will tend to serve secondary, complementary functions less efficiently. The primary function of military organizations is the coordination of large masses of men and materiel, often over continent-wide distances. The command of divisions and armies requires an approach that is inappropriate to the complementary but secondary function of R&D and innovation. If military commanders are tutored in a school that prepares them for careers as leaders of large, coordinated actions, their corporate ability to face the strong uncertainties of innovation will be impaired.

Other factors also intervene in the military innovation process. Size and complexity usually cause delays in innovation decisions and make the process an awkward one. The evolution of the organization is gradual, involving marginal changes and the successive addition and reduction of parts of the system and the weapons it uses. The organization will therefore be constituted at most times by both older and more recent weapons of varying technological vintages whose use is governed by an amalgam of doctrines.[2] In 1939, for example,

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[1]William Greene, *Strategies of the Major Oil Companies*, Ph.D. dissertation, Harvard Business School, 1980.

[2]These points are made by Zeev Bonen, Director of the Israel Armament Development Authority, in an unpublished paper distributed at the Massachusetts Institute of Technology, *Developing the Weapons of the 1980s*, 1976, p. 5.

the United States Army Air Corps had almost completely converted its first line bombers and fighters to modern monoplane aircraft with retractable landing gear and enclosed cockpits, whereas every shipboard fighter of the United States Navy was a wire-braced, fabric-skinned biplane.[1] (Germany at this time was testing the first turbojet powered aircraft.)

Because of the organizational rigidities of large, complex military organizations, the introduction of innovative technologies usually takes place in several stages.[2] In the first stage, new systems replace older ones, with little change to the organization or its doctrine. In most cases, the new system will be a marginal improvement to the older one; or if it is a new type, it has a greater chance of acceptance if it is independent, standing alone, imposing few perturbations on the rest of the system. An example of the latter type of innovation is the air-to-air missile. When first introduced, it had little effect on other aspects of the tactical aircraft mission, although United States Air Force proponents predicted close to revolutionary results from its expected high kill probabilities.

The second stage of innovation generally incorporates later, improved generations of the weapon, with some modification of organization and doctrine. The effectiveness of air-to-air missiles was gradually seen to be considerably smaller than originally thought, but it was also recognized that they had capabilities not possessed by guns: longer range and wider angles of attack. The tight flying formations favored for gun-carrying aircraft gave way to more spread-out, "loose-deuce" patterns that gave better offensive and defensive capabilities to the missile-armed (and missile-endangered) aircraft. These changes evolved during the 1965-75 period, largely in response to disappointing early experience in Vietnam.

The final innovation stage is characterized by organization and doctrine fully cognizant of and adapted to the evolving capabilities

[1] Robert Perry, *The Interaction of Technology and Doctrine in the USAF*, P-6281, The Rand Corporation, 1979, p. 5.

[2] The stages described here are those mentioned in Bonen, *op. cit.*

of the new technology, often integrated with other new technologies, systems, and sub-systems, perhaps accomplishing the original mission in novel ways.

Continuing with the air-to-air missile example carries us from historical example to proposed changes based on historical evidence, controlled experiments, and systems analysis. In the mid-1960s, the United States Navy in conjunction with Douglas Aircraft initiated the "Missileer" aircraft project. The concept behind this project carried the doctrine of air combat a considerable step beyond past practice and theory. A large aircraft, not intended for close maneuvering, was to have the ordnance capacity to "truck" a large number of air-to-air missiles into range of the enemy. This innovative concept called for a new type of aircraft, weapons, and doctrine of employment. Although the project was cancelled, analyses of air-to-air combat over the next decade, partly based on data generated from Vietnam combat experience and from experimental evidence of simulated combat (AIMVAL/ACEVAL), generated the concept of a "weapons platform": aircraft with powerful radar and other sensors, discriminating data processing capabilities, and long-range, all aspect, maneuvering air-to-air missiles with their own sensors. Such aircraft-missile combinations would perform the long-range phase of the air battle. The missile would replace complex and expensive fighter aircraft as the active, killing vehicle. Close-in defense would then be allocated to shorter-range, smaller, cheaper, more traditional fighters. If this stage of development actually occurs, it will have brought air defense by aircraft and missiles through several decades of evolution.

Decades-long adaptation periods are not unusual. Not only is the technology new, but the final form of the organization in which it is ultimately embedded is also new--the organization, its routines, and doctrines do not exist anywhere in the form they eventually will assume in the future. Therefore, the issues of strong uncertainties, experiments, feedback, and flexibility must apply to the using organizations as well as to the technology. No wonder it takes decades.

I have been assuming, until now, that the technology behind a weapon innovation is well in hand at the time the use of it comes

into question. This is not always the case; indeed, in the United States, and particularly in the United States Air Force since World War II, doctrine and use were often based on extrapolations of past technological trends. The R&D community has then been asked to respond to these requirements.[1] Although such extrapolations are seldom revolutionary, recalcitrant technology and premature decisions often blocked progress. Adherence to a philosophy of continual improvement of familiar forms virtually ruled out operational experiments to test the value of marginally improved weapons--experiments that might have demonstrated greater effects from improved training, tactics, or quantities than from the uncertified marginal improvements in range, speed, etc.[2] More importantly, this emphasis on improved performance of the known precluded trials of the innovative. Before 1954, the ballistic missile was the handicapped competitor against cruise missiles and manned bombers. Significantly, it took powerful outsiders (allied with a few enthusiastic insiders) to force ballistic missiles on a reluctant Air Force. In the ballistic missile case, truly revolutionary results came from a combination of technologies, several of which were pushed beyond their previous levels of attainment. However, the demands on guidance technology (which had been projected beyond technical feasibility) were relaxed when small-size thermonuclear warheads became possible. It was this tradeoff among technologies that made strategic ICBMs possible. This decision owed more to imagination than to analyses.

Premature attempts to introduce unready innovative technology into complex organizations can have chilling effects on its acceptance. This is as true of civilian as it is of military innovations.[3] The

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[1]This is the central theme of Perry, *op. cit.*

[2]Perry, *op. cit.*, p. 4.

[3]Examples of government sponsored demonstration projects that failed in civilian applications are described in Walter S. Baer, Leland L. Johnson, and Edward W. Merrow, *Analysis of Federally Funded Demonstration Projects: Executive Summary, Final Report, and Supporting Case Studies*, R-1926-DOC, R-1926-DOC, and R-1927-DOC, The Rand Corporation, Santa Monica, California, 1976.

best way to kill a project is to put an unreliable prototype into operational testing.[1] Changes in structure involving disruption of past roles and relationships produce apprehension and resistance, sometimes making the social cost of change larger than the monetary costs. Normal problems associated with new equipment are magnified and distorted by participants resisting the change. Immature products invite excessive discounts on the improvement of future performance. Adequate R&D is therefore essential to successful operational trials. The response of the United States Army in 1928-29 to the Christie tank illustrates this point. Infantry and cavalry promoters of tank innovations enthusiastically supported the designs of the American inventor J. Walter Christie. The Ordnance Department, however, rejected Christie's ideas in favor of their own improvements to models designed within the Army. Congress and the Army Chief of Staff then directed Ordnance to purchase the tank and conduct trials with it. This Ordnance did, but rejected the model on the basis of mechanical defects found while operating the prototype. The Army's strict insistence on Christie's meeting detailed specifications and reliability standards was ultimately successful in killing the project. The Soviet Union, in the meantime, purchased the prototypes and used them as the basis, over the next eight years, for developing the highly successful T-34.[2]

Given the ambiguity of information, the rigidity of organizations, and the difficulties inherent in new technology, is it possible to make efficient decisions about the choice of new technology and reduce the extended period to introduce innovations into military use? The next section makes some modest suggestions to improve this process.

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[1] Unfortunately, the best way to make a prototype reliable is to conduct field tests with it. This then calls for a series of trials--technical, operational, tactical, etc.

[2] Alexander, *op. cit.*, pp. 71-73.

V. EXPERIMENTATION FOR USE

The choice and use of new technologies must contend with strong uncertainties at least as numerous, as complex, and as powerful as those found in the development of the technology itself. Linkages between the sources of ideas and the users are critical to success in coping with these uncertainties. Just as the means for dealing with uncertainty in science and technology is through research and experiment, so it must be with the uncertainties of use. Experiments are kinder than warfare--mistakes send one back to the drawing board, not to the grave. But experiments can be dangerous in their own way--to established routines, missions, organizations, and budgets--and for these reasons they are often ignored or explicitly avoided.[1] Nevertheless, especially in the United States, the philosophy of quality weapons opposed to Soviet mass (and increasing quality) requires a systematic method for the introduction of innovative weapons, doctrines, and the organizations to use them.

I focus here on four kinds of experiments: (1) natural experiments (history, combat experience); (2) explicitly designed and controlled experiments (operational testing, experimental brigades); (3) paper experiments (systems analysis, operations research); and (4) mind experiments (imagination). Each of these types of experiments has strengths and weaknesses that complement the others. All are useful. None of them, by itself or together with others, will produce certainty; but they can reduce uncertainty.

Natural experiments generated the data used by Mahan and Clausewitz in their formulations of doctrine and their analyses of war. Mahan's best known work, *The Influence of Sea Power Upon History, 1660-1783*, covered a period that began 230 years before he wrote and ended more

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[1] "Sometimes I am amazed at the lack of attempts by people to find out what they ought or need to know *before* they start out developing equipment." This was said by the Artillery Test Director of the U.S. Army's Human Engineering Laboratory at Aberdeen after a series of tests that threatened to overturn conventional artillery procedures. R. B. Pengelley, "HELBAT: The Way to Tomorrow's Artillery?", *International Defense Review*, 1/1980, p. 83.

than a century earlier. His next book carried this history to 1812 and the end of the Napoleonic empire. These works illustrate many of the advantages of the historical approach, and also a critical deficiency. "A fundamental error" arose from his confidence in the "unchanging character of strategic principles."[1] Mahan failed to anticipate the importance of the submarine and torpedo, even though they were already developed weapons at the time he wrote. The critical deficiency of history is that it is backward looking. Developments beyond the period examined cannot, by the nature of history, be taken into account. Nor can other variables easily "be held constant," as modern analysts would say.[2] The very richness and reality of historical experience, which is its chief virtue, is also an analytical nightmare. How does one account for inept leadership, experience, terrain, morale, genius, and the other characteristics of real events? As we have seen above, combat experience can supply uncertain results, especially in short campaigns where battlefield feedback is limited. Nevertheless, warfare is the ultimate proving ground; despite the perplexities and riddles that reality imposes on analysis, it is only in warfare that one finds innovations tested in their contextual fullness.

Controlled experiments, in contrast, can hold other things constant. This is their strength. Whereas history asks the question, "What happened when...?", experiment asks, "What would happen if...?". Planned experiments can exercise weapons, organizations, doctrine, and tactics in a complex array of combinations, but without the full richness and reality of war. Despite the deficiencies of experiments, they have produced major benefits in the past, and are an underutilized technique today.

Experiments can take many forms. According to General Guderian, for example, the German army developed its doctrine of armored warfare on the strength of field trials in 1930 on dummy tanks made of sheet

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[1] Brodie, *op. cit.*, p. 277.

[2] I include under the historical approach empirically based, statistical studies of warfare. Given sufficient data, "military cliometrics" is sometimes capable of assessing the relative and independent effects of several variables. This approach seems to be used most often in examinations of force quantities and qualities.

iron set up on a wheeled chassis.[1]

In a similar vein, Eisenhower described his and George Patton's experiments with tanks at Camp Meade in 1919 and 1920. Noting the primitive nature of the vehicles in his possession, he commented that correcting its deficiencies "would require constant use in field maneuvers plus cooperation between military men and manufacturers."[2] Taking advantage of Patton's experience in France, they began a year of field experimentation in tactics combined with evening discussions of theory. When their tanks could not perform some desired mission because of technical limitations, they improvised by towing the tanks with trucks or by using the trucks alone. Eisenhower claimed that their ideas on equipment, tactics, and tank doctrine underwent continuous change with each day's trials. "In one respect, these circumstances were better than battle itself. Trial and error and the testing of alternatives is experiment and research--but in action, you are offered few second chances."[3] However, the rigid, official view of doctrine in this period was inhospitable to innovation. When Eisenhower and Patton began to write articles based on this experience, Eisenhower was called before the Chief of Infantry. "I was told that my ideas were not only wrong but dangerous and that henceforth I would keep them to myself. Particularly, I was not to publish anything incompatible with solid infantry doctrine. If I did, I would be hauled before a court-martial. George was given the same message."[4] Such was the official stance on issues about which no one at the time could possibly have had an informed understanding.

The use of experimental units is another technique for developing experience with new equipment. Organizationally separated from mainline units, they have greater freedom to play with tactics and doctrine in conjunction with the new equipment. When the Soviet Union first began to produce tanks on a large scale in 1930, they collected all

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[1] Cited by Bonen, *op. cit.*, p. 23.

[2] Dwight D. Eisenhower, *At Ease*, Doubleday and Company, New York, 1967, p. 156.

[3] *Ibid.*, p. 173.

[4] *Ibid.*, p. 173.

the tanks available to form an experimental brigade for collective trials prior to large-scale production.[1] They continue this practice today by introducing new equipment in pre-series production quantities to operational units. Early versions of the T-64 and T-72 tanks were first observed by Western viewers on large-scale maneuvers in 1972. Similarly, the Yak-36 (Forger)VTOL aircraft served on board a Soviet cruiser in limited pre-production numbers for some time before series production versions reached the fleet.

Training and maneuvers are other sources of experimental information. For training to be used in this way, it is essential that feedback be generated about the problems, successes, and failures encountered. Soviet training seems to have this characteristic, at least as viewed through articles in Soviet military journals that discuss the results of various units' attempts at solving problems during the training year. Large-scale maneuvers by the United States Army in the late 1930s provided direct evidence and experience with many of the new weapons and organizational formations that had been under development since 1918. For example, the importance of not separating tanks from their supporting elements was one of the organizational lessons coming out of the 1939 maneuvers.[2]

Carefully designed experiments, large-scale and small-scale, are another important technique for generating empirical evidence on war. The AIMVAL/ACEVAL aircraft-missile trials mentioned above produced surprising and uncomfortable information on aircraft and missile types, tactics, and doctrine. These tests are now having an important influence on debates about the next generations of aircraft and their armament.

Whatever the source, evidence is essential for the proper conduct of paper experiments. The enormous growth since World War II of systems analysis, operations research, combat modeling, cost-benefit analysis, and similar analytical techniques, has not always been matched by

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[1]Alexander, *op. cit.*, pp. 22-23.

[2]*Ibid.*, p. 69.

the data necessary to adequately carry out many of the studies. Nevertheless, such studies can be quite valuable in answering "what if" types of questions. The combination of facts and assumptions can establish the dominance or inferiority of prescribed cases; the analysis, however, cannot prove the truth of the underlying assumptions. The advantages of paper experiments are that they focus on central, abstracted relationships; they are relatively inexpensive; many combinations and patterns of variables can be analyzed quickly; and they provide means for holding other things constant. However, they are limited by the limited factual content of their parameters and by their sparseness and unreality--although some quite elaborate models have been built. Therefore, decisions on innovative technologies that may not be used for 10 years or more cannot be based on very detailed systems analyses or on war games. The major uncertainties inherent in the fundamental assumptions can only be reduced by actual work on hardware and field trials. Volumes of analytical work and greater specificity in the models cannot increase the informational content of the results. Field experiment and historical analysis though can support paper experiments by providing the necessary information to round out the models and specify the paramenters. A principal critic of paper experiments has acknowledged that although "much more emphasis must be placed on empirical work, and particularly on operational testing", this kind of analysis "likely has not achieved its fullest potential."[1]

Despite their usefulness, paper studies do not confront the principal feature of innovative behavior--strong uncertainties. Since a set of assumptions has been built into the analytical structure (either explicitly or implicitly), the outcomes of such studies cannot possibly be inconsistent with the assumptions. One of the chief analytical advantages of warfare and of operational experiments is that the assumptions can possibly be shown to be wrong in a surprising way. Nevertheless, the imaginative use of paper analyses can suggest what might happen if assumptions were different or wrong.

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[1] J. A. Stockfisch, *Models, Data, and War: A Critique of the Study of Conventional Forces*, R-1526-PR, The Rand Corporation, Santa Monica, 1975, p. iii.

Finally, we come to mind experiments--imagination. Not much need be said about this except that it is the foundation of all the other kinds of experiments.[1] One writer has even implied that innovative imagination will have its way despite the inadequacies of the other approaches. Bernard Brodie wrote that, "*in the long run*, technology has transformed war pretty much in its own fashion. The bumbling ideas of men about the utilities of new weapons have often caused painful and costly maladjustments, and have even determined at times which side would enjoy victory; but the mistakes that have been made in the past in these matters seem rarely to have affected the technological conditions in which men found themselves."[2] However, he goes on to note that the long-run can be very long indeed, and that in the meanwhile, events of grave moment "have their outcomes determined by gross errors of judgements on the significance of new military techniques."[3] Avoidance of gross errors of judgement is the joint task of those who originate ideas and those who use them.

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[1]A colleague is using imagination directly in his research by reviewing the scores of novels written about nuclear terrorism. The authors of these works have faced and solved in their minds many of the possible operational problems involved in both carrying out and preventing terrorist activities. In the absence of actual history, imaginary experiments provide a kind of evidence of potential acts.

[2]Brodie, *op. cit.*, p. 299.

[3]*Ibid.*, p. 300.

VI. CONCLUSIONS: LEARNING TO LIVE WITH UNCERTAINTY

Innovation is uncertain and risky, but failure to innovate carries its own risk. A conservative approach may require large jumps in performance at inopportune moments in order to make up for missed chances. Persistence in the conventional approach will surely result in a considerable lag behind technological potentials. If wartime exposes technologically backward systems and inadequate doctrine, it suddenly becomes necessary to carry out crash programs with reduced probability of success. It is therefore desirable to plan a systematic approach to experiment for use in peacetime--an approach that *generates* considerably uncertainty, surprise, and feedback, but that can reduce the occurrence of unwelcome and catastrophic surprise in war.

How can operational experiments and a more open, flexible attitude toward innovation be encouraged? I have already noted the many natural impediments to this course. But we must also recognize important attributes of military organizations that may help alleviate the problem. The very size and complexity of military establishments permit a heterogenous mix of organizations and activities to coexist. There may be niches between the primary organizations and their supporting routines in which sub-organizations with somewhat different approaches can play a role. Research and development commands and service schools exist in each of the services in the United States; they could properly take the lead in experimenting with new technologies and system concepts. It was the United States Air Force research and development community, for example, that promoted ballistic missiles inside the service, even though the Air Staff opposed the concept.

One way to promote a variety of views and organizational flexibility is to prohibit monopolies over missions and technologies. While this precept may violate the neatness sought by bureaucracies, it can introduce some needed competition in ideas, facts, and analyses. It was the Air Force that provided a key stimulus to the Army to review its decisions on the M-16 rifle; high-level decisions in the

Air Force and Department of Defense maintained competition among the three services in ballistic missile development, when the Air Force wanted monopoly power so as to downplay the emerging technology. The Marine Corps' search for an effective light-weight infantry combat fighting vehicle has stimulated the Army to maintain interest in several alternatives. The possibility that another organization could take over a mission can have an enlarging effect on what an organization considers possible or desirable.

Another approach is through the creation of explicitly designated experimental units associated with analytical organizations that could jointly design and carry out operational testing in controlled, realistic contexts. If permanent organizations were undesirable, *ad hoc* mini-organizations especially created to carry out specific tests and then disbanded following the tests is another technique that has been used in the past. The first possibility is represented by the Soviet experimental tank brigades of the early 1930s. They were permanent organizations intended to continuously carry out experiments in technology, tactics, organization, and doctrine. The second technique was used by the American Army in the 1930s, when tank units were formed for summer maneuvers and then disbanded several weeks later.

It is not necessary to think only in terms of sub-organizations within a service or other parent organization to generate innovations and to contribute to experimentation and analysis. The creation of Rand by the Air Force is an example of an organizational invention that tied a new unit to a parent body without binding the offspring with the same constraints and routines that affect the larger organization. In this way, different organizational goals and functions can be combined symbiotically. This could also be made to happen within a service, but it would require differentiated career structures and organizational routines that deviated from the standard patterns.

It should be noted that organizational design and all the human issues associated with military systems have been much less well treated by the research community than have hardware issues. To some degree, this reflects a weakness of demand for such analysis; but the analytical gaps make experimentation for use that much more important.

I have focused in this section on individual services and their sub-organizations while neglecting the overarching importance of high-level policy and attitudes. Official policy, as formulated by directives of the United States Department of Defense, are not generally consistent with the view of technology, weapons, and their use as experiments. For example, directives setting out the establishment of weapons requirements in the form of "Mission Element Needs Statements" (MENS) specifies that "the Secretary of Defense intends to satisfy the need identified in the MENS." Furthermore, when moving into the first phase of weapons acquisition, the directive requires assurance that the service "plans to acquire and operate the system."[1] Missing from these statements and others is any notion that R&D or operational test or further experimentation or a changing world may turn up information that could require a change in plans.

High-level policies must recognize that the use of new technologies and weapons is surrounded by strong uncertainties and that the appropriate means for dealing with strong uncertainties is through experimentation. The essential character of experiment in the realm of strong uncertainties is that the results are very likely to be surprising. The validity of concepts and the advisability of continuing must be re-examined after each stage. Planning should focus on the next stage. Projects should be structured as questions, not as assertions. A "Can do!" attitude must be replaced by, "Can do what?"

Elting Morison, with whom we opened this paper makes some suggestions as to how to proceed, despite the enormous complexity of existing systems, and the unillumined state of the future.[2] His central recommendation is to begin by scaling problems to life size. Whole systems cannot be designed and created from scratch. Concentration on manageable structures at least removes projects from the realm of the impossible. By proper scaling of the problem, interested parties can contribute their special knowledge while reducing the danger

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[1]Department of Defense Directive 5000.1, "Major System Acquisition," 19 March 1980, para. D2d.

[2]Morison, *op. cit.*, pp. 184-185.

of compartmentalization. Proceeding with specific projects encourages learning, confronts decisionmakers with real trade-offs, and supplies the evidence to evaluate the alternatives. "As the accumulating particular decisions move toward generality, a context is gradually assembled within which the parts and pieces and forces of the technological world can be fitted together."[1] This may not be the route to an optimized system with every part contributing maximally, but it is a prescription for a system that works. As Morison concludes, within the context pulled together from the accumulated experience and knowledge, leaders can act with authority, "rather than blunder forward, patching the leakage, damping down the explosions, adjusting the shortfalls."[2]

The fog of peace is incomparably more impenetrable than the fog of war. Experiment, adaptability, innovation, and change, in small steps first, can help pierce that fog. But in the end, it will always be with us, so that every step is experiment. The necessity of learning to live with uncertainty is perhaps the main conclusion of this paper.

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[1] *Ibid.*, p. 184.

[2] *Ibid.*, p. 185.

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